certain knowledge, 14, Lancaster Gate is supplied by the Grand Junction Company; thus Dr. Frankland has been analysing two samples of the same water, supposing them to have come from different sources. This mistake could not have been found out till after the report was printed, and the results obtained by the two experiments are as follow :—

| | Total solid impurity. | Organic carbon. | Organic nitrogen. | Ammonia. | Nitrogen as nitrates and nitrites. | Total combined nitrogen. | Previous sewage contamination. | Chlorine. | Total hardness. |
|---|---|---|---|---|---|---|---|---|---|
| Grand Junction, collected at the cab stand in Woodstock Street, May 17. | 24·7 | ·135 | ·020 | ·000 | ·184 | ·204 | 1520 | 1·62 | 19·36 |
| Water collected at 14, Lancaster Gate, supposed to be from the West Middlesex Company; but actually from the Grand Junction, May 15. | 24·6 | ·129 | ·023 | ·000 | ·188 | ·211 | 1560 | 1·60 | 19·36 |

It may be objected that even these numbers do not approximate so closely as those of Messrs. Wanklyn and Chapman, but they represent actual quantities obtained from water, and not a theoretical "albumenoid ammonia," which may not be an indication of the quantity of organic impurities.

Since Frankland and Armstrong's paper was published an immense number of analyses have been made with the process, and in his annual report to the Registrar General, Dr. Frankland states that he has seen no reason to be dissatisfied with the results. Probably no one, unconnected with Dr. Frankland's laboratory, knows better than Mr. Chapman that improvements in the details of the manipulations have been made during the last two years; and it is, therefore, with very questionable taste that he has reprinted the appendix to the first edition of the book without a single word of qualification.

It is much to be deplored that two young chemists, with such undoubted abilities as Messrs. Wanklyn and Chapman possess, should have rendered themselves notorious by attacking older workers in scientific investigation. It is, no doubt, very laudable in a young and ardent investigator, when he points out that high authorities may err and frequently have erred, but the manner in which these gentlemen have carried out their corrections has made their matter more distasteful. It would almost seem as if they found an incentive to work in the hope of being able to overthrow the "huge superstructures" which have been raised by men who have been longer in the field of scientific research.

## OUR BOOK-SHELF

*A Sketch of a Philosophy.* Part III. *The Chemistry of Natural Substances.* Illustrated by two folding plates and 150 figurate diagrams of molecules in the text. By John G. Macvicar, LL.D., D.D. (Williams and Norgate, 1870.)

It is a hard matter to give a just account of this pamphlet. The views propounded by the author are so entirely different from those usually held by chemists, and according to the author's own statement they have been so little studied by others, that it is difficult to know exactly how to treat the subject. We should scarcely be justified in saying that the whole system is mere imagination, though some hold this opinion; but the book, though evidently written with the intensest earnestness, is the work of an enthusiast, which will explain the bitter complaints he makes against modern chemists for not taking more notice of molecular morphology. The author endeavours to explain the formation of all matter by the aggregation of the ethereal element, supposing that all bodies tend to assume a symmetrical, and more or less spherical form. The simplest form of aggregation Dr. Macvicar considers to be the *tetrad*, consisting of four specks of the material element so arranged in space that they form the angles of a tetrahedron, the lines joining them indicating the attracting and repelling forces operating between the units. Two tetrads are also assumed to join base to base producing the *bitetrad*, and from these two forms the tetrad and the bitetrad, all the atoms and molecules of our planet are supposed to be produced. This tetrad by attracting another unit opposite to one of its faces constitutes a group of five units, considered to be the atoms of hydrogen, and with the atomic weight of five. The author proceeds to show the mode of genesis of many other elements and compounds by the juxtaposition of these elemental forms. By calculation he can determine by his system the specific gravities of solids and liquids referred to water as unity, in a manner similar to that by which the densities of gases and vapours may be deduced by the old system. This alone would seem to show that the method deserves more attention from chemists than it has yet received. The non-reception of this molecular morphology may be ascribed to several causes: the diction of the author is peculiar, and he writes in a dogmatic manner, which might be expected in a theological work, but is not usually found in a treatise on natural science; then he pushes his inferences to such an extent (or as some would say, rides his hobby so hard) that his conclusions appear somewhat ludicrous, unsupported as they are by experiment; thus he traces the coincidence between the assumed hexagonal form of the molecule of aqueous vapour and the shape of the minimum snow-flake and ice-flower, and "the inflorescence of plants of the monocotyledonous order, in which an aqueous tissue predominates"; he thinks that one of the forms of aqueous vapour which occupies half the volume of the other, may possibly be converted into the second variety at a high temperature, and thus explain the explosion of steam boilers. Again the dimorphism of water may be the cause of the production of animal heat; for water in the body may be transformed from one of its varieties into the other with evolution of heat; but on escaping from the body as perspiration, the inverse action takes place and cold is produced. But underneath all these extravagances there may be a stratum of truth, and we hope that either the author or some one who understands and accepts his views thoroughly, will so develop them as to ensure their reception by chemists.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

### The Source of Solar Energy

I HAVE not Mr. Proctor's "Other Worlds" by me to refer to, but my impression on reading that book some little time ago, certainly was, that if it did not directly support the *meteoric* theory of solar energy, it at least favoured the idea of innumerable meteors falling into the sun. The principal portion of my letter in last week's NATURE, was not, however, so much addressed against any special views of Mr. Proctor's relative to this *meteoric* theory, as it was against the probability of meteors fall-

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ing on to or into the sun at all. And whatever be the real value of the arguments I used, I certainly had not before seen them anywhere clearly stated. The question as to meteors being consumed in producing heat by friction in the solar atmosphere, or of their striking the sun's proper surface in a solid form, is of very secondary importance, and would of course be determined chiefly by the size and nature of the meteoric bodies themselves.* The potential or heat result theoretically would be the same either way; though in the former case it might hardly so well explain how whilst heated flames are shot up 60,000 or 70,000 miles from the sun itself, that body is also probably far hotter than the upper regions of this very atmosphere, in which the meteors themselves would be giving out heat.

Prestwich, Manchester, Aug. 5 R. P. GREG

### Kant's Transcendental Distinction between Affection and Function

AT page 80 of my "Revival of Philosophy at Cambridge," I ventured to describe a question set by Mr. Mahaffy, at Trinity College, Dublin, as "very oddly worded." I might have said *very improperly worded*, and have justified the sentence; but for the fact that it was the Cambridge examination-papers only that were the subject of my criticism in that work. My boldness in this censure on Mr. Mahaffy has occasioned remark. That gentleman is confessedly a capital metaphysician, perhaps of greater power than the learned professor whose work he translated and annotated. I therefore ask for space in NATURE to assign the reasons on which I asserted that his question was "very oddly worded." Here is the question: "Explain the statement that his [Kant's] doctrine of Space and Time is based on a *transcendental distinction*." I think I cannot be in error in taking this as a reference to the *Kritik der reinen Vernunft*, Transac. Æsth. § 8, *Allgemeine Anmerkungen*, &c., and in particular to the paragraph beginning "Die Leibnitz-Wolffsche Philosophie," &c., and that which immediately follows. In the former, and indeed in its immediate precursor, Kant is impugning the view that affection and function (Sense and Intellect) have only a logical difference, as if Sense were only differenced from Understanding by the inferiority of its representations, in precision and clearness. The latter paragraph (the third of those I have referred to) beginning "Wir unterschieden sonst wohl unter Erscheinungen das," &c., may be thus rendered:—

"We otherwise draw a proper distinction, in phenomena, between that which is an essential property of the intuition of it, and is generally valid for every one's sense, on the one hand; and on the other, that which belongs to it accidentally, inasmuch as it is not valid for the faculty of general sensibility, but only for a particular state or organisation of this or that sense."

This, Kant names an *empirical distinction*. To it he opposes what he calls a *transcendental distinction*, viz., that between phenomena and things in themselves, or what is involved therein, that between affection and function. As Kant points out, if we do not make the distinction between affection and function, we cannot explain the transcendental constituents of a phenomenon, and thereby we take it for a thing in itself, *i.e.*, a reality existing independently of our perception of it; and we so lose the distinction between the phenomenon and the thing in itself, of which latter we know nothing whatever.

Now, Mr. Mahaffy's question concerns a certain statement. Whose? Well, probably, it is his own, viz., that which occurs in a footnote to p. 57 of his translation of Fischer's Commentary on Kant's C. p. R. Mr. Mahaffy here says, "We must not confuse the *empirical* distinction between real object and merely subjective appearance with the *transcendental* distinction upon which his (Kant's) doctrine of space and time is based." This I believe to be the topic referred to in the question. It is manifestly open to two objections. Each of the terms "real object," and "merely subjective appearance" is equivocal. "Real object" may be the phenomenal object or the noumenal object. "Merely subjective appearance" may be what Kant calls "blosz Erscheinung" or the impression which the object makes on the particular sense of this or that subject. The actual terms used by Mr. Mahaffy more properly import the distinction between the thing in itself and its phenomenon; yet it is plain he means to follow Kant, and to speak of the distinction between phenomenal object, and particular subjective impression. It is the other matter of the extract with which the question deals; and it is this which I have no hesitation in pronouncing a very improper and misleading statement.

Kant's doctrine of Space and Time is *not* based on this distinction. On the contrary, the distinction is an outcome of that doctrine, and does not, cannot, emerge till that doctrine is established. The fruit of the doctrine cannot be its root; nor can that which is the basis of the distinction be itself based on that distinction. The truth is, that Kant's doctrine of Space and Time is one that concerns Sense only; it is Æsthetic; and as discovering the *a priori* sense-elements of experience, it is called Transcendental Æsthetic. Accordingly it touches but one pole of the distinction, and only so far helps it out! How can such a doctrine be properly said to be based on a distinction between the two poles? The very notion is preposterous, and derives, of course, no countenance whatever from the *Critic of pure Reason*.

The extremely curt and concise manner in which I have dealt with the actual subjects of examination in my book was essential to its brevity and corresponding lowness of price. The above remarks will show how insecure will be any inference of the poverty of my reasons from the paucity of my words.

Ilford, E., Aug. 1 C. M. INGLEBY

### Spontaneous Generation

IF there is one thing more curious than another in the "Spontaneous Generation" theory, it is the way in which so-called matters of fact, as proved by careful experiment, are brought forward by the one side to be disproved by the other; one need only instance Pasteur's famous flask experiments, which were thought to be so overwhelming at the time, but which were afterwards refuted, I think by Frémy and others.

I notice with surprise the letters of Prof. Wanklyn and Dr. Lionel Beale in NATURE, with regard to the presence of germs in the air; there is an experiment of Pasteur's, given in his "Memoirs upon the organised Corpuscles which exist in the Atmosphere, 1862," and which I have never seen disproved, and if not disproved it must surely settle at least this part of the question. He passed a quantity of air, taking various precautions to eliminate error, which I need not here detail, by means of an aspirator through a plug of gun-cotton; he then dissolved the gun-cotton in ether, and on examining the sediment which subsided in the course of an hour or two, he found abundant evidence of the presence of organised corpuscles.

Bath CHARLES EKIN

### Mirage

I HAVE just returned to England from H.M.S. *Porcupine*, having accompanied the dredging expedition as far as Lisbon. In reading the back numbers of NATURE, I notice in that for July 28 an account of an extraordinary mirage in the Firth of Forth on July 22. A reference to my journal shows me that on the same day we were dredging on the Portuguese coast, within sight of the Ferilhoe and Berlinga Islands, about forty miles north of Lisbon. The bearings of these islands and their exact distance, calculated by the aid of the known height of the lighthouse, gave us, of course, an exact position, which our "dead reckoning" also confirmed. Several solar observations, both for latitude and longitude, were taken by two of the officers during the day, both of whom always arrived at the same result, but this was so widely different from our position as previously determined by two other methods, that we were forced to the conclusion that there was a very false horizon. It was the only instance of the kind during the month I was at sea.

Clifton, Aug. 8 WM. LANT CARPENTER

### The Sun's Corona

IF "my mathematical result was based upon data among which the principal point at issue was accepted as proved," it will be easy for you to state what that point is,* and to quote one passage at least in which Mr. Lockyer has associated it with his theory. In this way alone can you justify the assertion in your last editorial note.

So much you are bound in common justice to do. But further, it would be satisfactory if a distinct statement of Mr. Lockyer's

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* Suppose the case of a large mass of meteoric iron falling through the solar atmosphere, the first result would of course be a development of heat and light; but if the original or cosmical velocity of the meteoric body was lost through the resistance of the atmosphere (as in similar cases terrestrially happens) *before* the entire mass was consumed, there might possibly result an actual loss of solar energy, caused by the subsequent and necessary melting of the residuum.

* A possible action at the moon's limb suggested by Faye, Gould and others.—ED.

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